4 Supplemental Comments

Workshop invitees and participants were provided contact information to forward any additional comments regarding the workshops or the future of Ocean Exploration. The workshop facilitators made every effort to incorporate all supplemental comments into the workshop tables found in Section 3, Workshop Results, of Volume I. The remaining comments not incorporated in Volume I have been sorted by region and reported below.

Caribbean Region

Future of Ocean Exploration

Ex• plo• ra• tion: To search or range over for the purpose of discovery.

Dis• cov• er• y: The initial act of finding or observing.

In his book, "The Coming of the Golden Age: A View of the End of Progress", published in 1969, Dr. Gunther Stent argued that the period of intensive, basic scientific investigation is coming to an end because science will soon have solved enough societal problems that life will be too cozy for tax payers to justify supporting the increasingly expensive experiments necessary to answer the last remaining difficult science questions. John Horgan, in his recent book "The End of Science", argues similarly, that most of the tough questions about our world and universe have been answered and what is left is simply filling in the decimal places.

Perhaps this view has merit as it applies to many fields of physics, including hydrodynamic modeling and global ocean circulation. This view, however, is a bit optimistic in as far as our knowledge of biological processes. True, we have started to unravel the mysteries of the genetic code, but we are nowhere near using that information to define all of the possible forms of life that can result. One could also argue that through quantum physics, we have the ability to describe any possible chemical reaction. However, the total number of all possible reactions is so large that we could not possibly investigate them all systematically and our knowledge base is too small to even narrow down our investigation to a manageable subset of the most important reactions. There are no theories to guide us in these questions.

How do the related roles of exploration and discovery factor in? The former is the determined pursuit of the latter and the latter is the catalyst for new theory and models. So, new ideas of our world and universe must come from new exploration. To say that we have formulated the definitive model, and therefore the end of scientific enquiry, is to say that we have observed every important condition, or at least the vast majority of all possible important conditions, within the model domain and that these observations agree with model predictions to such an extent the underlying theory must be accepted as fact. Without on-going exploration, we make the assumption that the probability of observing a uniquely new condition that does not agree with the recognized model is very low. This would be the scientist's view of Stent's societal cost-benefit analysis of scientific support. This, for example, is the situation in modeling global ocean circulation where we have a high degree of confidence in the equations of state and model accuracy is a function of our ability to define the boundary conditions. It could also be argued that we are approaching this situation, if we are not already there, as it applies to marine ecosystems that are based on photosynthetic primary production. While there is a great deal of disagreement between model results and observations, the underlying equations are probably correct and our weakness is in our poor knowledge of rates. In other words, the theory is sound, but we lack the required detail to make accurate predictions.

So, what role then does exploration and discovery have in oceanography?

We have described in detail only a small fraction of the global ocean. Of the 78% of our planet covered by ocean, most detailed oceanographic knowledge concerns a small fraction of the total ocean volume, representing surface, sun-lit waters, several hundred meters in thickness, and confined primarily to the exclusive economic zones of the worlds coastal nations. Globally, our knowledge of the ocean at depths below 500m is poor to nonexistent. While we would expect the basic laws of hydrodynamics and quantum physics to apply to the entire ocean volume, the same is clearly not true of biology. Take, for example, the unique life forms that have evolved around deep sea vents independent of solar energy. The life cycles of these organisms are completely unlike anything that we had observed prior to their discovery. Likewise, we know little of the important chemical reactions that take place in this environment of super-heated water and immense temperature gradients. As new technologies have emerged that enhance our ability to observe and survey the deep ocean, the rate of discovery has increased. Several times a year, for example, we hear of a unique, previously unknown, life form.

The first goal of a U.S. strategy for global ocean exploration, articulated in "The Report of the President's Panel on Ocean Exploration", is to map the important physical, geological, biological, chemical, and archeological features. It draws no clear distinction regarding the relative importance with regard to discipline nor does it identify specific geographic areas of the ocean on which to concentrate. These are important questions because we will always be limited in the amount of funds available for exploration. The metric for comparison is, in my opinion, the probability that an observation will be made that so completely disagrees with current theories and models that it triggers a re-evaluation of the underlying theory. The model can be anything of societal importance – environmental, economic, or strategic. Since we are dealing with the unknown, one way to guesses at such probabilities is to examine the number of observations made so far relative to the number of important discoveries.

Let me address each of these disciplines separately. Since I have argued that most of our oceanographic knowledge is derived from near-surface measurements, I will assume that any mission of discovery will concentrate on the deep ocean where our basic knowledge is poor. I will assign a numerical score to the number of observations (O) that have been reported in the open literature; 1 = few or no observations, 5 = enough observations to question a theory, 10 = enough data to construct and validate a model. I will also assign a score for the number of recent discoveries (D) that serve to excite the research community and/or society in general; 1 = few to none, 5 = a few over the past several decades, 10 = several each decade. The cumulative score then will be computed as D/O. The higher the cumulative score, the higher the priority within a global ocean discovery project.

Physical processes, the first discipline mentioned in the President's Panel report, would score a medium in observations since many CTD casts have been made into the deep ocean and we have at least some knowledge of deep-ocean currents by way of drifters. While the topic is important to global climate modeling and we do lack details regarding deep water mass formation, published observations have so far not confounded basic principles of hydrodynamics. One could argue that there may be small-scale processes associated with deep-sea vents that we have not thought of, but such processes can be reproduced in the laboratory and investigated in detail should the interest arise. Recent observations that call into question accepted models, such as Dr. Broecker's deep-sea conveyor belt, suggest that we need more detailed information about deep ocean circulation in order to adjust current models rather than to conduct a complete rethinking of the basic underlying theory. Therefore, O = 5, D = 1 and CS = 0.2.

Geology is the second mentioned science discipline, perhaps due to the potential economic benefit associated with any discovery. For similar reasoning, the international oceanographic community has directed significant funds to the investigation of deep-sea geology. The NSF Deep Sea Drilling Project, for example, currently directs $12M annually towards the collection and analysis of deep-sea cores. Discoveries in the past 50 years have greatly changed our ideas regarding the composition of and the geological processes occurring at the deep ocean floor; e.g., sea-floor spreading, plate techtonics, and the discovery of manganese nodules. Therefore, I assign the following scores: O = 10, D = 10 and CS = 1.

Most biological oceanographic research has been directed towards the near-surface, sun-lit areas of the ocean in support of fisheries commerce. Relatively few observations of deep-sea biology exist and what does exist is rich in information that has changed our view of life in the deep-sea and the range of

conditions under which life can exist. Information gained in the harsh conditions of a deep sea vent, for example, shapes our ideas about the origin of life on Earth and the possibility of life on other planets. And yet, the large percentage of new information in each deep-sea biological data set suggests that much more information is needed in order to adequately characterize the deep ocean. Therefore, O = 1, D = 10, and CS = 10.

Chemistry is next on the list in the President's Panel report. In many respects, chemical observations and discoveries are similar in nature to those in biological oceanography. Again, the most interesting observations seem to be in association with deep-sea vents; inorganic-based reactions that lead to mineral deposits and bio-chemical reactions that make life in that extreme environment possible. The number of discoveries are not as numerous as in biological investigations, but just as thought provoking. T suggest that O = 1, D = 5, and CS = 5.

Last on the list is archeology, our window into past human endeavors. Of all the disciplines listed, this one has the highest potential to stir societal interest. In fact, most of the funds that support archeological research come from private foundations and donations from private individuals. It is also the only discipline in which results rarely influence our future actions, only our view of the past. Archeological results may turn anthropological theories on their head, but even the most radical paradigm shift will be of little or no consequence in our present day-to-day activities. Most notable recent finds have emerged from Dr. Bob Ballard's expeditions to the Mediterranean Sea and the Black Sea. These observations have forced us to rethink our ideas about traditional trade routes at the dawn of history and the origins of myths and biblical stories. And yet, the ocean floor remains largely unexplored. Therefore, O = 1, D = 5, and CS = 5.

This analysis would suggest the following priority ranking, starting with the highest;

Biology	10
Chemistry	5
Archeology	5
Geology	1
Physics	.2

Given the availability of funds, perhaps concentrating on the highest priority issues is most appropriate. An ambitious science goal, for example, would be to simply catalog the abundance and diversity of life from the base of the sun-lit layer to the ocean floor. In my opinion, that would be about as ambitious as President Kennedy's 1961 proposal to congress to land a man on the moon by the end of that decade and the results would have a much higher potential to impact how we view our own planet and, perhaps, how we treat it in the future.

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Ocean Exploration suggested topics

1. Make four (4) new posters each year based on one or more projects. Include URL at bottom of poster.

Photo galley converted to a screen saver format. Download the new files - place a watermark with the Explore logo in the corner.
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2. Create access point for the OE program at **libraries**. The poster can be place above a terminal with Internet connectivity. This promotes understanding and recognition of the program among the general public

3. Plan a DVD archive and a DVD publication of underwater, atmospheric explorations. Film the "filmers" and other support persons on an exploration to promote secondary careers.

4. From the DVD publication series, compile a teacher/classroom package. Schools would love to have video related material. Schools cannot use the on-line access for video because the band-width is limited to the classroom.

5. Difference between basis research and ocean exploration

Basic research the outcome is dominant	OE the journey and personalities
science peer group	OE general public
complex, technical	OE direct, simple

6. Yardstick for success Number of People know about the program, common knowledge among a wide spectrum of general public

7. Outreach is essential Media, education,. Theme park links, museums, cruise ships and other affinity groups

8. Pathway from OE to basic research Track an idea developed in OE and then migrates to the professional science domain.

9. Partners in OE Academe Commercial NGO

10. International Governments Academe Tourist industry

11. Remote observations Web based access to real time explorations

Issue is bandwidth at remote locations US internally is no problem

12. Explain how the oceans/atmosphere operate within one theme.

13. Radio spots produced for NPR and other broadcasters.

14. Writer's conference for adventure writers to have access to the NOAA Explore assets and resource material

15. Next year (calendar year 2003) invert the primary themes and cross cut themes Tech and Outreach prime, then ocean dynamics and mapping as crosscut.

16. School kids to design explorer project like the science in space program at NASA has.

17. List the NOAA operational programs and NOAA science programs to the Explore topics funded or planned. Link the promotional to the functional as a progression to service to the public.

18. Create an *"Explorers' Digital Log"* as a publication vehicle for the observations as they are captured from the various *in situ* instruments, cameras and other direct methods of exploring. The log would be considered a digital publication so a citation would be created. The library would be ideal place to host the

service - a video screen, projector for large screen applications and other routing to NOAA offices at Silver Spring. Live feed into all conference rooms fromthe library would add new dimension to library services.

19. At future workshops capture the profile of participants by affiliation, category (government, academe, NGO, etc.)

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As we discussed on the phone, I was at the IOCARIBE (the IOC regional subcommittee for the Caribbean) meeting during your regional workshop in Miami. As a long-time physical oceanographic researcher in the Caribbean, and regional project coordinator for the Caribbean regional Ocean Observing System (IOCARIBE-GOOS), I have a few thoughts on potential Ocean Exploration projects in the region. Probably the most interesting, and highly relevant to Ocean Exploration objectives, would be a study of the active undersea volcano Kick 'em Jenny located about four miles north of the Island of Grenada. I have done two opportunistic surveys of the cone in 1996 and 2002, the latest with the Ron Brown's multibeam system (figure attached). The cone rises from 1500 m to within 180 m of the surface; the 1939 and 1974 eruptions sent debris above the sea surface. Given the active nature of the region (e.g. Pelee on Martinique in 1902, Soufriere Hills in Montserrat from1996 on), not only is KEJ likely to be the next Caribbean island, it poses a significant potential hazard to life and property throughout the region. I have been talking to volcanologist Haraldur Sigurdsson of the University of Rhode Island, who conducted studies of the volcano in the 1970's, and the University of the West Indies Seismic Research Unit in Trinidad about in-depth scientific studies of the volcano. I imagine Coral Reef exploration and mapping was brought up - this is definitely a priority regional issue, perhaps best suited to OE by exploring one of the more obscure regions using new remote sensing technologies along with in situ observations. You already did the connectivity issue last year; that's one of my pet interests and I wish we could have been involved earlier in that program. I am studying transport of waters through the Caribbean Passages. One of the major unknowns is what is happening in the circulation of the deep Caribbean. The Caribbean is a series of isolated basins which have trapped overflowing North Atlantic Deep water over a long time period; there is probably a lot of climate history here in the chemical and tracer composition of these waters, which are very poorly sampled. In particular there is little known about the deep overflow (or other transports) in the Windward Passage between Cuba and Haiti. I have been working with the Cubans and we did the only existing deep transport section there in 1996, there needs to be more long-term studies. Understanding role of the Caribbean on climate, North American Monsoons, and hurricane formation is needed, but not sure is OE material. Issue probably brought up and of OE interest is understanding of Caribbean whale populations, from Dominican Rep. through Eastern Caribbean not much known, potential for acoustic remote sensing.

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Hawaii Region

"... We are very interested in the new OE program (especially if we can get you interested in the territorial waters around Samoa!..."

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I realized that I had promised to send in some comments I got from local scientists to factor into your distillation of the workshop input. I think some if not most of what is included was mentioned, but I wanted to be sure that I followed through.

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Combined Comments from Some Hawaii Scientists

EXPLORATION NEEDS

1. We need to explore any areas left that haven't been explored and mapped, for relationship to current or future needs. For example, search for previously-unknown fissures, mineral deposits, lesser tectonic plate edges that could be involved in future earthquakes, new sealife or plant species or large numbers of existing species, etc.

OPPORTUNITIES

1. Assess quantities of species as a baseline for future comparison.

2. Establish environmental baseline for the world's oceans, to aid in future evaluations.

3. Establish a "belt" within the 200 mi. limit for controlled exploitation; i.e., mineral deposits, sand, power generation, offshore aquaculture, and medical/pharmaceutical solutions.

4. Select a site under the seafloor, offshore an unpopulated area for future nuclear waste storage. A site away from tectonic plate activity, that we can tunnel out 30 miles or more, and create a more acceptable storage than Nevada, or Alaska. It would create an economic boom for some area, maybe in the Gulf, or off Mexico or Canada.

5. Establish manned habitats on the seafloor which, like the space labs, will generate new technologies and interest in ocean exploration. The habitats should be placed in areas that need periodic monitoring, so records can be kept for environmental comparison.

PRIORITIES

1. Establishing a baseline for environmental change-monitoring, now and a century from now. This will be vital.

2. Establishing a nuclear waste site also is a vital need, and should be started soon.

II.

Briefly: far more satellite pop-off and acoustic tagging of fish needs to be undertaken in Hawaiian waters. We don't know where many of the fish important to Hawaii (Pacific blue marlin, swordfish, various tuna, giant trevally, sharks) come from, go to or do in the course of an average day/week/month/year. Without that information, we can't properly manage them. The technology exists and is well proven, what we need is the financial commitment to place thousands of high-tech tags in various species.

III.

One aspect of proposals and discussions with respect to ocean exploration is the lack of provision for taxonomy/systematics of marine organisms. Taxonomy/systematics in general gets tacked on to some biology courses, but rarely if ever is recognized in the curriculum I am appalled by the lack of interest of doing anything about learning names and identities of marine organisms -- or even how to go about putting names on what one finds, both within marine biology departments/ programs and among graduate students. I hope that in the proposed initiative there is opportunity for some concerted action to provide funding for the taxonomy/systematics that will be crucial to making sense our of the organisms that are going to show up in the proposed explorations.

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I felt obliged to voice some developing concerns about an important set of ocean observations that may be overlooked. Long term heat flow measurements on the ocean floor extending hundreds of kilometers normal to, and on either side of, rapidly spreading ridge axes (or hot spots) to determine the lateral extent of the heat "footprint" during intense episodes of sea floor spreading (or hot spot volcanism) that are current
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or may have occurred hundreds or thousands of years ago. Also of interest would be long term ocean-wide monitoring to see if there are correlatable ocean-wide fluctuations in heat flow. These might be related to core-mantle interaction that happened hundreds or thousands of years ago and are now just reaching the surface. Regardless of any theoretical speculations, long term measures of fluctuations in sea-floor heat flow may be a very important yet poorly understood data base.

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Maritime archaeology notes

1) folks seemed very interested in pinpointing locations. Shipwrecks are different from studying fish or widespread invertebrates. We have a list of around 200 commercial shipwrecks, and are currently working on a list of over 136 US navy ships and aircraft in the Hawaiian waters. Many of these are pinpointed by lat/long numbers already. It would have simply taken too much time to begin listing these wrecks site-by-site during the workshop. Additionally, the naval historical center is very careful (and often rightly so) with distribution of coordinates for its properties. As we have a grant from them to create an initial inventory, I would really have to check with them before releasing locations for wrecks.

Known locations prioritize targets...there are WWII submarines, torpedoed navy tankers, etc. in Hawaiian waters, each with a story to tell. None, though, have been confirmed by going back, diving on the numbers, and groundtruthing the target since its sinking. If I had to prioritize areas, the list would be something like Midway atoll environs (aircraft), Alenuinui channel between Hawaii and Maui, dumping grounds SW of Barber's Pt Oahu, historic defensive zone outside Pearl Harbor entrance, areas near shore to Lahaina, Honolulu Harbor, Hilo Bay, Waialua Bay, etc etc.

When targets came up in discussion during the workshop, people began naming individual items or wrecks which they had happened to hear about. To immediately jump to that scale of analysis is silly.

2) Smithsonian Institution (if not on the list of associated agencies for this topic) should be added. Dr. Paul Johnston has already done some work in Hawaii, curator of maritime history at the American History museum.

3) funding sources, after jotting down the basics, others piped up with "Discovery channel" and "National geographic." There is a reason those sources are not usually listed by professionals who are in the preservation and maritime history/archaeology field, and that is that a lot of survey and study does not revolve around media agenda. Popular perceptions of maritime archaeology include media sources, and oceanographers **…**, but beware of jumping to conclusions that [this] is representative of the field. **…** Real research designs are often slow, careful, and not geared towards producing daily flashy results for the cameras. There is nothing wrong with that stuff per se, but maritime archaeology research must be driven by professionals who have been trained in the field, or it will not be taken seriously by anyone except for network programmers.

4) Prioritization of targets is not really random, but something proscribed by federal preservation legislation, this is something quite different from the scientific method approach. Significance is defined fairly clearly by a number of federal documents. Spending money to look for a wreck which is not historically significant, even if it might be appealing to the media, can be seen as a waste of time. Have someone on board your planning team that knows preservation legislation, national register criteria as applied to ships, maritime history, etc.

I thank you for your emphasis on interdisciplinary multi-tasking exploration, for including non-scientific topics like maritime history and maritime archaeology. I'm quite interested to see how it really goes down.

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Alaska Region

Exploration Emphasis Area Identification and Prioritization

In terms of exploration, obviously the areas that are least explored are those that are more challenging to get to either logistically or economically, i.e. ice covered seas and areas that are served by "third world" ports. I suggest that these areas be the focus of the OE Program - areas where new discoveries are most likely to be made simply because the amount of prior information is sparse. This is not the easiest path, but it is the one that most directly addresses the aim of the Ocean Exploration Program. We already invest a great deal in research in US waters; this initiative can broaden our reach to a truly global perspective.

While I appreciate the political and economic challenges associated with working outside our EEZ, and especially in waters where the political climate is less stable and friendly than might be wished, I would point out that oceanographic processes do not recognize political boundaries.

Continuous water masses have currents, biota, contaminants, etc. that pass freely and interact regardless of anthropogenic lines. Our artificial division of these systems has minimized our understanding and slowed progress immeasurably ? while we understand pieces of near-coastal systems we will never have an integrated understanding of large scale processes unless we address them as such, not as bits that act in isolation. This is an especially pertinent issue in oceans like the Arctic and Southern Oceans where many nations have political interests. I am not suggesting that we risk scientists and resources in piratical waters (like the nearshore Gulf of Aden), but that if we cannot address systems holistically, simply for political reasons, that we put our resources elsewhere where we can do the science correctly. For example, in the near-coastal Arctic, we need studies that address poorly described regions near Russia - not more studies of the same areas that we know relatively well in US waters. Only with expanded coverage can we begin to address regional issues that are unique and pertinent to a fairly isolated ocean basin like the Arctic. If politics alter the path of the science, our support should be moved to areas where we can pursue the most relevant questions, not wasted in doing marginal work out of some misguided conception of "fairness." For Ocean Explorations to succeed and provide the most and best return for the investment, the science needs to drive it, not the politics.

Identification and Characterization of Exploration Strategies

The first need that any explorer has is for a map. Without this, discoveries cannot be placed in context nor communicated to others. The map may be a traditional bathymetric one, or a map of species diversity or of genetic variability, the commonality is that it provides a baseline for comparisons across space and time. "Mapping" does not compete well in traditional research proposals, yet its importance is fundamental. As the OE Program offers an opportunity for exploration (which also does not compete well in traditional funding routes), I hope it will also recognize the essential importance of maps and baselines to exploration, and to all science.

There is much technology that is currently available for oceanographic work, but it is underutilized by the scientific community because of cost and competition for scarce resources (e.g. the manned submersible Alvin and the remotely operated vehicle Jason are extremely oversubscribed, with funded researchers often waiting 2 years for field equipment time). Making these kinds of major resources available, either by supporting construction of new tools or by providing funds to rent commercially available items, would enhance scientific exploration. I would emphasize support for existing technology that is functional but unavailable, rather than development of new technology that in general requires a very long term and extremely large investment of funds before utility and functionality are achieved. There are many routes (commercial and defense) that support technology development. Much more can be done with existing tools, if they are made accessible.

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"...We are very interested in your program, however, as it relates to the discovery or exploration of submerged archaeological and historical resources in Alaska waters..."

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Good meeting you in Anchorage. I think the meetings were very productive. I only wish more (than one) biologist had been there. Attached is the figure I circulated showing age and spatial coverage of NOS hydro surveys in the eastern Bering Sea. The data were assembled with the assistance of Coast Survey's Don Haines and Steve Verry. LCDR Doug Baird called the other day to confirm that there is in fact a 780 nm2 hole (Bristol Bay) that has never been formally surveyed and added that the 1950s vintage surveys (most of the rest of the open water area) are at the 1:500K scale.

One final point. Although my EFH interests are pretty clear, I would like to suggest that the Bering Sea Fish Habitats Info Need/Gap should be broadened to reflect general exploration, this being an area with considerable national economic and global environmental (WWF designation) significance. As stated, the issue seems too parochial and agency-oriented. Also, please modify the Technologies section to clarify that interferometric side scan sonar (true swath bathy and true backscatter) is key to seabed characterization in highly-structured and variable areas of general low relief.

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"The main argument for OE studies in the Aleutian arc region (to include the deep trench on the Pacific side, the volcanically active Aleutian ridge proper, and slope and deep water regions on the Bering Sea side) come from the recognition that tectonic and volcanic activity in the region ensure that a single transect across the arc will encompass environments that are enormously diverse both physically and chemically. It is the physical and chemical diversity of these environments, and the near-complete lack of prior exploration in the region, that create the unusual opportunity to discover isolated and previously unrecognized biological communities in close geographic proximity."

Many thanks for your attention--I hope this short statement can help promote the Aleutian region as an area of interest to OE.

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This letter is to convey follow-up comments about the Alaska Regional Workshop.

As I studied the workshop report, three matters bubbled to my attention. One was an operational matter, and two were thematic opportunities that I did not think were exploited enough.

1) The operational or technical matter concerns the description of the "Standard Package". It seemed to me that for an exploration-discovery type program, which I like very much, that an essential part of the standard go-to-sea package should include a digital single channel seismic reflection system. A high resolution (portable) rather than a low-frequence system is what is needed. A signal-clean, gas-injector or water-gun source would be fine. Digital recording is a must so that seismic processing software packages can be used to clean up the records and extract acoustically useful field data.

2) Going to the first thematic matter, I thought that although a few words were said, I did not get a feeling that adequate emphasis was placed on hydrothermal vent hunting to locate isolated biologic communities and sea floor mineral masses. For example, many of the pull-apart basin located along the volcanic line west of, say, Kiska are likely habitats of hydrothermal activity. Exploration of Buldir and Ingenstrem Depressions, Prochoda (sp) scarp and related basins might prove exceptionally rewarding target areas for having a look. Years ago I recovered undatably young hornblende dacite from the northern crest
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of Ingenstrem Depression. So, the transform fault system out that way bleeds magma, some (maybe a lot) of which is "adakite" in composition. I smell black and white smokers everywhere.

3) With respect to exploratory studies of hydrate deposits, the central area (~3700 m water depth) of the Aleutian Basin (Bering Sea Basin) is underlain by virtually thousands of massive accumulations of methane hydrate. The hydrate masses in the sediment and the underlying columns of ascending thermogenic methane that feed them make for rather spectacular acoustic anomalies on seismic reflection records. The anomalies are called VAMP structures (Velocity-AMPlitude). The main mass of hydrate is several km in diameter, maybe 250 m high, and extends downward to the BSR (bottom-simulating reflector) located at a subsurface depth near 450 m. Reflection horizons overlying the massive hydrate deposits are blanked and broken up a bit, so it may be that methane is feeding through the gaps to the sea floor to nourish a benthic community. This is pure wonderment on my part, but taking a high resolution look at the sea floor above a VAMP structure could prove rewarding.

It's pretty clear that a great deal of methane is sequestered beneath the Aleutian Basin at VAMP structures. But no exploratory work has been carried out to map what one of these bodies looks like in 2D, let alone 3D. Although many thousands of km of digital seismic data are available (taken by USGS during the GLORIA cruise), no VAMP, as far as I know, has been crossed by two lines. The subsurface geometry of a VAMP structure is thus unknown. Mapping this geometry would be a juicy OE product with just all sorts of implications scientific and political (i.e, EEZ boundaries with Russians).

So, for the purpose of assessing the resource potential of VAMP structures (some VAMPs appear to involve a quantity of methane equivalent to that of a large gas field), their potential linkage to deep-water biological communities, and the amount of global-change gas stored in the Bering Sea Basin, exploration efforts to finally learn something definitive about VAMP structures would be a first-order program contribution. One can get started with planning this sort of work by calling upon the USGS archive of seismic data, which is large but has never been used to study hydrate deposits. Extracting information from this existing data set would be a great foundation for a masters thesis, the results of which would serve as the template and guide to conduct a cold-blooded exploration of one (or more) of these massive hydrate accumulations.

It is true that DOE is currently sponsoring a variety of efforts to explore and study methane hydrates, and this could be seen as a disincentive for Ocean Exploration to do hydrate-related exploration. However, virtually all of DOE's supported (and jointly with industry) investigations are for the Gulf of Mexico. This is true because industry is concerned about geohazards linked to shallow hydrate deposits in the gulf, where the oil and gas industry has a large financial stake. Because US industries do not consider (at least not yet) marine hydrate deposits as a resource (i.e., as a source of economically producible methane), nor are hydrates a global-change concern to them, and because US oil and gas companies, within the US EEZ, are not exploring for offshore resources outside of the Gulf of Mexico, DOE is accordingly not devoting research dollars for hydrate studies outside of the Gulf.

This circumstance leaves open a very large research-focused window of exploration opportunity for the EEZ of the Pacific, Bering Sea, and Arctic Alaska regions. For discovery investigations concerning large masses of hydrate, the Bering Sea is the place to go. Something interesting to note is the circumstance that hydrate deposits are commonly found (drilling and BSR studies) in slope deposits around the rims of most ocean basins. But slope sediment is in general pretty soupy stuff and lacks permeability and good reservoir characteristics. But if you can locate hydrate deposits in the sedimentary sequences of ocean margin basins, for example the Bering Sea Basin, the Sea of Okhotsk, and Sea of Japan, then you have a chance to accumulate hydrate in reservoir quality fan and basin-floor turbidite deposits. Things get very exciting if the basin-floor section is thick enough to produce thermogenic (petroleum) methane. Where this happens, which is the situation for the Bering Sea Basin, then exceptionally large quantities of methane hydrate can accumulate in the shallow subsurface.

Anyhow, for program planning purposes, consider that the opportunity to conduct hydrate exploration studies is wide open to NOAA's OE. Neither DOE, the USGS, nor the NSF is interested

(except, perhaps, for the Office of Polar Programs). The Japanese, however, might be interested because they are keen on discovering producible methane deposits along the Pacific rim.

I truly hope that NOAA's OE program does well. When a program's mission is to explore frontiers, discoveries will happen--again, again, and again. It's rather sad that my old organization (USGS) turned away from this critical element of their public mission. As you know, except for some minor efforts, the ocean floors of the EEZ, in particular for the Alaska offshore, have been abandoned for the shorelines of the lower 48 and Hawaii. I'm just happy that NOAA has picked up the challenge to make discoveries in the offshore waters both shallow and deep.

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TRANSECT ACROSS THE ARC

I am really intrigued by concept of doing one or more transects across the Aleutian Arc plate boundary, as discussed at the workshop. I think this is very compelling as a framework for an interdisciplinary OE expedition -- in a dynamic environment, in an area of the US EEZ that is ripe for exploration. In fact, there are very good reasons to do either two or three transects (explained below).

The transects would cross from the Pacific Ocean into the Bering Sea, starting from structures on the downgoing Pacific plate and crossing over to the backarc in the Bering Sea (the limit of active volcanism, north of the volcanic islands and beyond the limit of tectonic activity). There are clear candidates for exploration in deep trench biology, structural geology, volcanology, potential for hydrothermal venting, biology and physical oceanography in the shallow passes between islands, etc. During the workshop we mainly discussed the middle part of the transects, i.e., the volcanic arc, the Aleutian Ridge, the shallow passes between the Pacific and the Bering Sea, the deep rifted canyons. To elaborate on other features that could be crossed by transects:

structures on Pacific plate: Large structures on the Pacific plate just south of the Aleutian Trench have been identified in GLORIA images. They are interpreted as fault blocks created by bending in the Pacific plate as it enters the trench. These structures may be associated with seafloor fluid seeps and/or volcanism. Bob Duncan et al. found "young" volcanics on Patton Smt (western Gulf of Alaska) during ALVIN dives in 1999; Patton Smt is in a similar setting, though farther from the trench, and Duncan et al. think the volcanism there has been triggered by deformation as the seamount approaches the trench. Could be included in the south side of "transects."

flank failures & debris slides: The north flanks of five (out of six) volcanoes along a 135-km section of the central Aleutian arc have fallen into the sea. From west to east: Tanaga, Boborof, Kanaga, Adagdak, Great Sitkin. The scars are evident in the shape and stratigraphy of the volcanoes on the islands, and large debris fields out on the floor of the Bering Sea can be seen on GLORIA images. The circumstances of these flank failures are unknown, except that they are not related to caldera collapse - they are classed as debris avalanches, and may have caused tsunamis when they occurred. When did they occur (how closely spaced in time, and how catastrophic?). What was the trigger (some tectonic event such as enormous earthquakes, or sudden tilting of the islands?). This topic was not mentioned at the workshop, but is one that the Alaska Volcano Observatory geologists are actively interested in. It is also a topic that bridges between the Aleutian Islands (with which people are more or less familiar) and the unknown region offshore. Could be included in the north side of "transects."

cold seeps & hydrothermal vents: The high likelihood of cold seeps in the Bering Sea and along the southern margin of the Alaska Peninsula is paired with an equally high likelihood of hydrothermal vents in the Aleutian Arc. This unusual geographical proximity presents an opportunity to compare the biology at hydrate-driven cold seeps with biology at nearby arc-driven hydrothermal vents. Many (most?) of the organisms at hydrothermal vents are thought to have gone extinct in the late Cretaceous, and then re-radiated out from cold seep environments -- biologists say that the two are genetically and evolutionarily related. But this is based on studies at mid-ocean ridges and continental margins, geographically very far apart. Is there a closer (and possibly more recent) evolutionary relationship, or possibly even direct
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exchange, where the two settings are in close geographic proximity? This is something that was brought up at the workshop, but I'm not sure the idea got across.

comparison of transects across the eastern, central, and western Aleutians: Several major characteristics of the physical environment change dramatically from east to west along the 2500 km long Aleutian Arc. The most obvious is the convergence angle between Pacific and North American tectonic plates, controlling the volume of volcanism and amount of tectonic dismemberment; hence the whole character of the arc changes systematically from east to west. Another is the volume of sediment input to the trench. Sediments from the North American continent are transported westward along the trench approximately as far west as Amlia; beyond that, the trench seems to be sediment-starved. I don't know enough about the biology to say, but I suspect that it varies with distance from the major landmasses (and fishing ports), if nothing else. The forcing functions vary so strongly along the arc that doing a transect of just one locality misses the potential of the Aleutians. The eastern part of the arc is at least closer to the main North American landmass, and hence has seen at least some exploration. The central and western parts of the arc are big question marks; studying these areas would advance the general state of scientific knowledge about one of the most under explored parts of the plate margin system.

native community input: The central part of these transects would of course cross the shallow Aleutian Ridge, where the Aleuts have traveled, fished, etc. for thousands of years. Involving them as advisors and participants in exploration of this area would be a natural way to recognize this segment of the population. This is a sensitive issue in AK.

VERY DEEP DIVING VEHICLES - JASON II

During the discussion about exploration of the Aleutian Trench, JAMSTEC was mentioned briefly as a potential collaborator. There did not seem to be any special interest in working with JAMSTEC, only a recognition of the need for very deep vehicle capability. However, after the meeting I was reminded that the new JASON II has a 6000 m depth limit. I would much prefer to see OE rely on JASON II for studies in the Aleutians.

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West Coast Region

I'm interested in data that exist or might be gathered on the abundance of species occupying subtidal rock substrates in Washington and Oregon, especially benthic invertebrates. This specific interest stems from a more general interest in effects of isolation by distance on colonization and persistence of populations.

Thus far, I have found no references to published surveys of representative sites but haven't yet searched technical reports. If this habitat is as understudied as it seems to be, it is a remarkable gap in knowledge of biota of the continental shelf.

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Workshop ideas for Outreach and Education

I attended the Monterey workshop to represent education interest in OE planning. Thought we didn't discuss education to any great degree there was widespread support and inclusion for it. I did manage to talk briefly with the OOE staff but since then I have some additional ideas, thought and suggestions.

One suggestion that came out of our group discussion, which I strongly encourage, is the idea of holding an OE education summit(s) and planning session. I also strongly recommended that in the future the education summit should happen at the same time and as part of these regional workshop so there could be an exchanged and sharing of the ideas and needs between the educators and researcher. I think having separate workshops perpetuates the "its not our thing" mentality. I personally got a lot out of listening and

being apart of these workshop sessions. I think researchers would also get a lot out of listening and being apart of some education planning sessions. In my opinion it shouldn't be just one educator like myself at these workshops or one researcher participating in an education summit session. Ideally this would be a combined workshop when there is an assemblage of educators and researchers. It could be as simple as adding another break out session focused on outreach education and having the ideas presented to the whole group.

There is no doubt that the OE program is committed to education but there is a need for clarity on what the education strategy is and who is the audience. If the need for the program is to gain public support then the educational strategies and audience maybe different from the traditional K-12 approach. Clearly this program can't address all the educational needs or ideas. What is needed, however, is to figured out where the program can make the biggest difference and contribution toward ocean science education while at the same time raise awareness and support. I think an education summit(s) could help identify this kind of focus as well as harvest the many great ideas and needs similar to what is being done in these workshop.

Another aspect of OE education that I thought about after the meeting is the challenge the PI's have to include outreach and education in their proposals. Often it is thrown in at the last minute or is an assemblage of ideas or just a very basic and traditional outreach approach. Outreach and education is becoming more and more important for competitive proposals. It seems that there are a couple of approaches that OE can take with this. One approach is we (education community) all work individually with PI's and get included on their proposals. For the most part this seems to be what is happening now. Another approach might be to figure out a more coordinated way to include outreach and education that serve the needs of both the PI and educators. I don't have any specific ideas but it might be an interesting discussion to have with the researchers, educators and the OE staff.

I understand that there are educators on the proposal review panel. This is great and I am curious how the outreach and education components are weighted and if a criteria has been developed. It seems like more involved efforts should score higher than less complex traditional O&E. This might be a contribution that OE can make for other proposal review process if it hasn't already been developed. I believe that with this increasing requirement for O&E in proposals is developing a need and an opportunity to provide researchers and the education community with new skills, resources, services, networks, and coordination.

Other thoughts and ideas include:

-Use the education funds to leverage and match more funds from other agencies or NGO's.

-Develop an auditorium presentation about OE and the various projects that can be distributed to public facilities with auditoriums. Maybe provide small grants for purchasing hardware for those places that are willing to include this into their programming but may not have the system to support it.

-Develop a kiosk that present OE and the various projects that can be mass produced and placed in aquariums, visitor and science centers, school lobbies or banks and building lobbies. This kiosk would serve as an brief attraction to the program and direct people to the website and other resources to explore more. These kiosk can be produced inexpensively in quantity and could be widely distributed for no cost or low cost. The kiosk could also be designed to continually update itself by accessing data files from a single or multiple server sources. These could be stand alone units or if accessible to a phone line connected to the web. The kiosk could also be part of the trade show style display, part of a table top display, or it stand alone.

-Education programs and service need to work or be included in the designing of the Data Archiving system.

-If OE wants to reach teachers then resource are needed to pay for release time, stipends etc.

-Create a simple exhibit development kit that could be used by classrooms, small interpretive center, and even large aquariums. This kit has instruction to build a table top or larger exhibit. It also has options and costs associated for each type with step-by-step plans, materials list and access to text and image materials. Perhaps these can be made available through the web or pdf files. If higher quality images are needed maybe the OE can provide the images, maps, etc. The OE could also provide advertising and marketing materials to promote the exhibit or display for those who participate and develop one of these displays. The kit could also provide templates that can be printed off, traced, cut out, and used for fabrication. The History of Ocean Science on the web site is a good example of the source material for exhibits and displays. Perhaps small grants could be made available to smaller interpretive centers. This kit could be designed to be adaptable for each project and PI's could include this in their proposal as a part of the O&E package. The small grants could go to those groups who are committed to working with these PI's.

Develop and electronic curriculum that has access to parts of the data from expedition and creates a free choice learning (i.e.. exploration) of Ocean Science and is used in a structure learning environment (i.e.. formal education) The electronic curriculum sets up learning objective but how they are met is up to the students own exploration verses the teachers directing a whole class activity. The teacher facilitates exploration and discovery. The OE program would be a natural subject to experiment with this kind of approach. This is also a blending of informal and formal education.

Use facilities like the Aquariums and Science Centers as NODES to reach a network of schools, marine educators and interpretive centers. Coastal America has a network call Coastal Ecosystem Learning Centers. The strength of these centers are the regional connections and networks to other groups and programs. OE might be able to provide resources to develop and strengthen these links and networks by sponsoring regional networking workshops or supporting the development of a consolidated resource directories of education and OE research for each region.

Hold student ocean conferences at these nodes or in the Ports from which the expeditions travel through. We just held a Coastal America Student Ocean Conference that focused on Marine Protected Areas. This was a successful event with 50 student. An OE student conference could be done in a manner such that students could meet with the scientist for an expedition debrief session. If OE is planning to have public and media events as it has in the past this could be an added element to these events. OE would need to provide some funds for coordination of the event and student travel (in some cases) to attend.

Hopefully some of this is useful and it makes sense. I would be glad to talk to you more if you have any questions, need clarification, want to hear more about the meeting, or discuss ideas.

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North Atlantic Region

Where is the Line Between Exploration and Research?

NOAA's new Ocean Exploration program presents exciting opportunities for unimagined discoveries and understanding of the ocean realm. Traditionally, most discoveries in the ocean have been serendipitous encounters made during the course of research funded through normal funding channels. For example, discovery of hydrothermal vent communities was made during geological investigations of ocean spreading centers. New species are routinely collected during hypothesis driven work on marine communities or during routine characterization of various parts of the ocean. We now have an opportunity to conduct ocean voyages focused on discovery, an opportunity not widely available for nearly half a century.
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Proposing and designing projects for such voyages can be confusing for the research community and problematic for program managers. For example, what agency and program should fund such proposals --when should a proposal be sent to OE, versus NURP, versus NSF? For the sake of both the individual programs and the scientific communities they fund and represent, all extramural programs should articulate a dividing line (or perhaps more realistically, a gray zone) between OE activities and those of other national programs (e.g., NOAA's NURP, NSF's SGER). Such groups in the past have routinely supported both exploratory and hypothesis driven research.

Where does exploration end and hypothesis driven research begin? Webster's Seventh New Collegiate Dictionary defines explore as:

> a "search through or into, to examine minutely especially for diagnostic purposes, to penetrate into or range over for purposes of geographic discovery, to make or conduct a *systematic search*" and research as.
>
> "*careful or diligent search*, studious inquiry or examination, investigation or experimentation aimed at the discovery and interpretation of facts, revision of accepted theories or laws in the light of new facts or practical application of such new or revised theories or laws."

There is no clear division in these definitions and one could look at exploration and research along a gradient of systematic inquiry.

The President's Panel on Ocean Exploration defines exploration as "… discovery through disciplined diverse observations and the recording of the findings." Further, the report distinguishes an explorer from a researcher " … by virtue of the fact that an explorer has not narrowly defined the observing strategy to test a specific hypothesis." However, the description of exploration that follows goes full circle by adding, "[a]nswering questions and following up on ideas will still be cornerstones of the new program." Further, the report states that "[w]hile hypotheses may be less specific, and their outcome less predictable than the current norm, the observations will be more broadly based, and the program more interdisciplinary." The end point here is clearly that hypothesis driven work will still be supported, albeit perhaps based at least on straw man hypotheses, and provides little insight for defining a boundary.

So, how do we proceed in defining exploration and its place in federal ocean programs? One direction could be based on a division between observation and experimentation. Observational programs, those that seek to define pattern in the natural world, fit nicely in the realm of exploration. This leaves experimental studies, those requiring manipulation and replication, to the traditional funding sources. Another direction could be based on the specificity of hypotheses. Both avenues seem to present a scale for finding a dividing line. How do we advise investigators where such a line is, and more importantly, is this a line we wish to draw? From the perspective of NURP, we have supported all types of scientific inquiry: studies defining pattern based on systematic observation; experimental studies based on manipulation and pattern recognition; experimental studies based on a priori hypotheses; and both pattern recognition and experimental research based on hypotheses that are narrowly defined. The ability to construct a priori hypotheses simply reflects knowledge of existing paradigms such that new discoveries can be set within some scientific context. Narrowly defined hypotheses can be viewed as a reflection of more specific knowledge such that null and alternative hypotheses are based on a clear set of narrow predictions.

The multidisciplinary nature of "exploration" as defined by the President's panel perhaps can provide a key and dividing line that may aid in directing investigators to one program or another. However, this will also require understanding of the context of "multidisciplinary" studies. For example, does a cruise with specialists in community ecology, population genetics, and behavioral studies, all areas of biology, constitute an interdisciplinary study or must the science party have a biologist, geologist,

physical oceanographer and meteorologist? Given the wide breadth of any discipline, I posit that there are few individuals who could be ready to identify all of the possible serendipitous discoveries in any particular region of the ocean. In any case, exploratory work should involve some level of multidisciplinary team to meet the gauge for an "exploration" proposal. Something may also be gained by emphasizing the distinction between multidisciplinary and interdisciplinary-- the former, within the context of this discussion, implying a cruise with a wide range of disciplines in order to "cover all the bases," while the later implies a group of scientists working on a single project from multiple perspectives. The former being the nature of an exploratory team, while the latter being more akin to modern oceanographic research teams.

This leads us to the issue of systematic observation as the fundamental tenet of voyages of discovery. Just as NASA's planetary explorations by humans and robotic vehicles field teams of scientists to develop a set of observations that can be made within the limits of spacecraft weight, time, and money, so too can oceanic voyages be planned and implemented within such a context. We can identify areas of the world's oceans where little is known and develop targeted cruises to collect sets of samples and data. The types of collections and methods required for such collections can be made by a wider group and implemented by the science party at hand. Perhaps scientists (and others such as educators and artists) can rotate through the cruise legs, allowing full participation by those willing to invest time and energy in such endeavors. The products of such cruises can then feed a range of disciplines and allow the scientific community to use the results of these cruises to better understand pattern and process in the ocean. Collections could be focused on characterizing broad biological, geological and physical oceanographic characteristics at each station and improved high resolutions maps could serve as guides for subsequent experimental, process-oriented, hypothesis-driven research.

There is purposely no conclusion here, although I have my own opinion. This leaves us with questions that require consensus to answer: Can we articulate a difference between exploration and research? Perhaps more germane, do we want to articulate such a dividing line? We need to consider our constituents but we also need to consider how such decisions could affect the ultimate directions of our programs.

A logical next step, however, could be to utilize the upcoming Ocean Exploration workshops as critical venues to engage the ocean community in further refining these distinctions. Just as the Oceans Commission is traveling around the nation, meeting and discussing with a broad set of users to set the future directions for all of the oceans, so too can Ocean Exploration reap the collective wisdom of hundreds of dedicated members of the community to help forge the direction of Ocean Exploration in the coming decades.

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Great Lakes Region

Generally film footage of lake bottoms is always useful.

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Here are a few thoughts on Great Lakes cultural resource explorations. I was unable to make the trip.
Great Lakes Unique factors

- Relatively small area
- Not deep by ocean standards

Extraordinarily high density of historic submerged cultural resources. For many decades the worlds busiest industrial waterway.
Excellent site preservation (threatened by environmental/human activity)
Major periods of historic significance as far as the density of archaeological remains 1820-1920.

Density of wrecks reflect complex processes in North American history. Best analyzed part of a series of interrelated chapters in North American History.
Some suggested historical themes
Native American Cultures
The Fur Trade
International Military Action
Westward Migration
History of Technology (diffusion of technology/environmental affects on technological design)
Industrial America
More than in other regions cultural resource explorations must be managed.
Isolated or individual programs of shipwreck discovery threaten to destroy the cultural resources that are needed to create the new questions that are at the heart of the discovery process.
Priorities
1. Environmental assessment of shipwreck sites. How is the environment affecting preservation? Key issue, factors affecting site stability This is necessary to prioritize explorations.
2. Native American/Paleo-Indian Sites. A largely untapped area.
3. Pre-1870 passenger steam vessels
4. Schooners
    a. 1800-1820
    b. 1820-1845
    c. 1845-1865
    d. 1865- later
5. Bulk Carriers 1869-1900
6. Non-Shipwreck coastal sites (mills, docks, etc.)

How.
1. Regional Exploration/Survey
2. Data Mining
3. New Techniques for low/medium cost non- or minimally destructive archaeological investigation
4. New methods for site monitoring and protection/ Site stabilization
5. Curation and conservation facilities and technologies.
6. Identification of regional experts.

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South Atlantic Region

"...Per your recommendation, I am submitting written comments that address needs, opportunities, and priorities for exploration of coastal and oceanic regimes."

Needs

Understanding and predicting how living organisms thrive in the ocean interior are difficult tasks. Future progress in understanding biological processes in the deep ocean will depend on in situ investigations that carefully organize objectives and deploy novel tools and technologies in representative environments. It is well-recognized that traditional, remote approaches to study the water column phenomena, i.e., deploying instrument packages from ships and using satellites and buoys to monitor changes, are blind and need to be sea-truthed. There are at least two ways to remedy this fundamental problem. Conduct short-term, small-scale investigations by using undersea vehicles to conduct detailed observations and pertinent experiments. Establish long-term, multi-scale explorations by developing undersea observatories to monitor critical events.

More cross-disciplinary transfer between physical, chemical, and biological oceanographers is essential. For example, it has been clear for a long time from in situ observations that the oceans are multi-layered, in physical, chemical and biological dimensions. The "new" discoveries of thin layers made with innovative profiling technologies that are deployed from ship or autonomous platforms need to be sea-truth with in
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situ interventions. Such research would guide the development of predictive models about topics such as biodiversity, food web dynamics, carbon transport, remineralization, bioturbation, and diagenesis.

Greater use of manipulative field experiments are required to record rates and fluxes in deep-water environments in order to define mechanisms that regulate dispersal strategies, e.g., fluid and hydrophylic variables that serve as cues for larval transport and settlement.

Intervention with manned and robotic vehicles capable of recording high-resolution images and conducting controlled experiments are needed to investigate episodic events (e.g., harmful jellyfish blooms), invasions of exotic species (e.g., food web disruption), and particle transport processes (e.g., large aggregate flux).

Opportunities

More frequent and more widespread opportunities to use of undersea vehicles to enter ocean environments (=depths beyond those possible with scuba diving) are clearly necessary. The lack of extensive support for both direct access to the uppermost 2000 m with undersea vehicles and for technical improvement of diving platforms capable of operating to 2000 m has limited ocean exploration for decades.

Priorities

General:

Provide greater access to ocean environments with manned and robotic vehicles.

Support extensive development of state-of-the-art instruments (e.g., cameras, sensors, lasers) and tools (e.g., devices to samples biological, chemical and geological materials) that can be deployed with undersea vehicles or moored at specific undersea locations.

Specific (One subset of the projects that should be given priority):

Map biodiversity, vertical distribution and relative abundance of gelatinous zooplankton (i.e., ctenophores, siphonophores, medusae, salps and appendicularians).

Identify intra- and interspecific trophic relationships of gelatinous zooplankton.

Investigate life history strategies (e.g., fecundity, growth rates, overwintering, prey selection, advection) for the most numerous gelatinous zooplankton species.

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South Atlantic Region

Atlantic Ocean, south of Hatteras

Just a few thoughts:

1. Gas/Oil Reserves and Conservation: NC and US has a region (this is right on the edge of north and south of Hatteras divisions) with potential for oil/gas deposits. Some of the tracks have been leased, but no exploratory drilling has occurred. This is in the EEZ, just on the continental slope, near the confluence of the Gulf Stream and Labrador Current, known The Point. Questions have been raised about the stability and sediments of this area, benthic life, and currents. Some oceanographic research has addressed these points, but not much. If this area becomes open to drilling, it would be a good idea to have some more data. Education should have more information and visuals about this area from the surface and upper water column abundance and biodiversity, to benthic life to sediments in order to allow students and citizens better access to data, options.

2. Non pelagic living resources: The deep coral growth and the “hard bottoms” on the Gulf Stream side of the continental shelf are fairly unknown. Some of these area which are in 600 feet of water and more, are
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possible spawning grounds for some grouper species. The extent of the hard bottoms, outcrops and corals (cold water, no zooxanthellae) would be fascinating for extending student knowledge of scarcely known ecosystems.

The book, Ship of Gold in the Deep Blue Sea, by Gary Kinder raises all sorts of questions about shipwrecks and identification/salvage technology in deep ocean off South Carolina to Bahamas. Some of the deep water mollusks (slit shells) and sponges have different life styles from shallower creatures which would be interesting if good videos were available.

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Deep environments along the Florida Reef Tract merit consideration for exploration. Of particular interest are potential sites for reef fish spawning aggregations, which may need to be evaluated for additional protective measures. A good example is Riley's Hump, south of the Dry

Tortugas, which has been protected within a marine reserve since July 2001 (Tortugas Ecological Reserve, South: http://www.fknms.nos.noaa.gov/tortugas/currentplans/implementation.html).

We know little about sites for spawning aggregations in and near the Sanctuary.

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